Statistical Design for the Evaluation of Cloud Seeding in Minnesota

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by

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Executive Summary - Statistical Design for the Evaluation of Cloud Seeding in Minnesota.

Minnesota's 1977 weather modification law stressed the need for evaluation of operational (commercial) weather modification activities in the State. This paper examines the problems of obtaining valid quantitative evaluations of cloud seeding projects, particularly projects attempting to increase rainfall. Here a quantitative evaluation means answering the questions, "Can one reasonably conclude that the effect of seeding was not zero?" and "What is a good estimate of the effect due to seeding?". Ultimately the paper presents several plans of operation, known as designs, that allow for valid evaluation.

Experimental cloud seeding began in 1946. Thirty years of research, although certainly fruitful, have produced few definite results. It is generally conceded that under suitable conditions a single cloud can be induced to produce rainfall. However, experimental attempts to increase rainfall over a specified area by seeding groups of clouds have not produced conclusive or even consistently positive results. Detailed analyses of several large experiments by the Berkeley Statistical Laboratory have led to the current "working hypotheses": Cloud seeding intended to affect rain over a moderate sized area can have no effect, but it also can

1) have strong effects, either positive or negative;
2) have effects that extend over unexpectedly large areas, perhaps up to 200 miles from the point of seeding;
3) have effects that last over a longer time period than expected, especially into the day after seeding.

Most importantly, meteorologists cannot now predict under what conditions these effects will occur. This uncertainty in the state of the art is

nowhere greater than for the summertime cumulus clouds of the midwest.

The first question the paper addresses is whether a valid evaluation can be obtained from the usual operational program where all suitable seeding situations are in fact seeded. The conclusion was that such an evaluation was not possible. This was for two reasons: First, a control group of observations needed to serve for comparison with the seeded group must necessarily be chosen from historical records. Research has shown that any such method is open to uncontrollable biases putting any conclusions in doubt. Second, a model is needed for a statistical analysis of the data. Meteorological scientists cannot yet provide an adequate model. Consequently, the recommendation is that the State expend no effort to attempt to quantitatively evaluate the usual operational programs.

If the State wants to allow for valid evaluation of cloud seeding projects, it must require operators to seed in a manner that conforms to the principles of experimental design. In particular, this would require letting roughly half of the suitable seeding occasions go unseeded to serve as a control group. The minimal requirements for implementation and analysis of such an operational project are:

1) careful consideration and definition of seeding method, suitability criteria for initiating seeding, area and time interval for measurement, and system of measurement;
2) a method for randomly selecting the control group;
3) a full-time inspector to insure unbiased selection of suitable seeding occasions, correct randomized selection of the control group, and consistent recording of measurements;

4) all projects within 200 miles of one another must follow one seeding schedule because of the possibility of contamination;

5) no use of a suitable occasion, either to seed or not to seed, that follows within 24 hours of seeding because of the possibility of contamination;

6) knowledge of statistics at least equivalent to a master's degree and considerable computer time.

The paper discusses these requirements in detail. Requirements 1 and 2 will be stressed here: The choices made for the first requirement may be critical for detecting any seeding effect that exists. Also, to achieve an adequate sample size it may be necessary to combine the data from more than one project. In that case the seeding and measurement methods must be consistent. The second requirement removes or reduces the problems in the analysis of the usual operational program.

One further point must be emphasized. The above requirements for an operational project guarantee the possibility of a valid quantitative evaluation. However, they do not guarantee that any seeding effect will have a reasonable chance of being detected. For example, the usual effect claimed by cloud seeders is a 10-20% increase in natural rainfall. Suppose that seeding causes a 20% increase and a project runs long enough to have 100 suitable seeding occasions. (This would require about 3 summers of seeding.) In this case the chance of detecting the effect is only about 20%. Because of the insufficient state of meteorological knowledge the only certain way of increasing the detection probability is by increasing the sample size. It appears that at least 200 suitable seeding occasions are needed to achieve a reasonably high chance of detecting a 10-20% seeding effect. It is highly doubtful that any one project could provide enough observations. Consequently the primary hope lies in combining results from separate projects and this requires, at the minimum, the same seeding and measurement methods in each project. In the face of the possible inconclusiveness of the results the cost of insuring valid evaluation may be unacceptable to the State.

In summary, an adequate evaluation can only come from a carefully designed and monitored operational program. Furthermore, the State must make a long term commitment to insure an adequate amount of data.

0. Introduction

The theoretical basis for modern cloud seeding methods has its origins in work done in the years 1911-1945. In 1946 when V.J. Schaefer discovered that dry ice dropped into a supercooled cloud in a chamber produced millions of ice crystals, the means for testing these ideas became available. In the same year Schaefer dropped 3 lbs. of crushed dry ice into an alto-cumulus cloud deck at about -20°C. On the ground Dr. Irving Langmuir observed snow falling and evaporating approximately 2000 ft. from the base of the cloud. Also in 1946 B. Vonnegut discovered that small crystals of silver iodide in smoke form acted efficiently as ice-forming nuclei if the temperature was below -5°C. Subsequently, General Electric scientists produced the first clear evidence that silver iodide smoke particles could change the state of a natural supercooled cloud. Meanwhile in Australia in 1947 Kraus and Squires seeded large supercooled cumulus clouds with dry ice and obtained indications they had produced rainfall (Mason, 1975). Since then many experiments on individual clouds have been performed and it is generally conceded that both dry ice and silver iodide can induce a suitable cloud to produce rain (Moran, 1970).

However, the question of whether seeding can produce a detectable increase of rainfall in a specified area is still entirely open. Since the early 1950's there have been many commercial operations that have claimed to have increased precipitation, but for a variety of reasons these claims have been discounted. Considerably fewer in number, the randomized research experiments have presented no consistent results. Consequently many meteorologists and statisticians have adopted the working hypotheses that under some conditions seeding increases rainfall, while under others it can decrease rainfall or have no effect. Furthermore, the effects may cover a much larger area and extend over a longer period of time than originally expected (Neyman, 1975). As stated by Professor Roscoe Braham, head of Project Whitetop, an unsolved problem is that "... meteorologists at present do not have sufficiently detailed knowledge of clouds and cloud processes (nor adequate seeding agent delivery systems) to permit specifying beforehand which effect will result." (Neyman, p. 316, 1967).

Given the uncertainty of effects as well as the legal, social, and economic implications, the Minnesota Legislature deemed it appropriate to pass a bill allowing regulation of commercial (operational) weather modification by the Department of Agriculture. Part of the import of the bill is to insure the possibility of an adequate evaluation of any cloud seeding activity in Minnesota. This paper addresses the problem of how cloud seeding activities can be conducted so that valid statistical inferences can be drawn from the measured results.

In a statistical analysis of any sort the manner in which the observations are taken is crucial to the defendability of any conclusions. As will be explained later, in the face of current meteorological knowledge an evaluation of the usual type of operational program produces only moot results and those after painstaking if not unending analysis. The reason for this is that the usual operational program does not employ a statistically valid experimental procedure. This is certainly understandable since valid procedures require some suitable days to remain unseeded and it is doubtful that consumers would buy the services of an operator who insists on not seeding a certain percentage of the time. Nevertheless, from a statistical inference viewpoint some form of valid experimental procedure which employs a randomized selection of the control group is necessary. At least two large operational programs in California have used a randomization plan (Elliott, 1974).

This paper will present several statistically valid plans of operation, henceforth called designs, which employ a randomization procedure and are in theory applicable to both operational programs and research experiments. To properly motivate the randomization intrinsic to these designs it is necessary to first explain what problems an analysis of a usual operational program faces and how a randomized design alleviates them. The structure of the paper is as follows:

The first section will discuss what inferences one wants to make in an evaluation of an operational program. It will present the difficult problems and assumptions inherent in evaluating any nonrandomized experiment. In particular the primary method for analyzing operational programs will be shown to be even more suspect than one might expect from general considerations. Specific areas of bias will be listed. This section will serve as a caution should the Department of Agriculture decide to evaluate the nonrandomized operational programs in the State.

The second section will give the philosophy and logic for randomization. This entails showing how randomization helps solve the problems described in the previous section. Since the need for randomization is not entirely conceded or understood, the presentation will be as complete as possible without becoming mired in fine points.

The third section will be a description of one of the most important randomized cloud seeding experiments, Project Whitetop in Missouri. It was chosen because it comes as close as any large experiment to Minnesota's climatic and geographical conditions. It is also an example of an excellently conducted cloud seeding experiment.

The fourth section will be the detailed discussion of the recommended designs. The general questions of definition of treatment, definition of experimental unit, and appropriate measurements, including those of hail and downwind effects, will be examined. The use and desirability of concomitant observations will be noted. Each design will be carefully described with a reference for an example and an appropriate analysis of the data. The strengths and weaknesses of each design will be emphasized.

The final section will review the important issues and provide general recommendations.

This paper will hopefully serve three purposes: First, should the Department of Agriculture decide to force some or all cloud seeders to use statistically valid designs, this paper could be a guideline for the regulations. Second, if the state of Minnesota enters into any research or operational project this paper may be a valuable part of the pilot study. Finally, this paper acts as a warning of the problems involved in designing and analyzing a cloud seeding project.

1. Evaluation and the Nonrandomized Operational Project

The assumption of this paper is that evaluation of a cloud seeding project is concerned primarily with answering two questions: The first is whether or not seeding produced (caused) any effect in a given area for a given time period. The usual effect of interest in the Plains States is a change in rainfall from the natural, although it could also be a change in hailfall. The area of interest is usually a specific target; but there is also concern for the areas around the target, particularly those downwind. The time period is usually the duration of a storm, although again there are questions of a "persistence" effect over a longer period. The second question is how to estimate the effect and gage the precision of the estimate. We will consider how to answer these questions after a typical operational program.

For the sake of illustration we describe a usual operational program as occurring in the following manner: In the midst of a dry period a group of farmers, businessmen, and local officials raise sufficient funds to engage a commercial cloud seeder in the hopes that seeding will enhance natural rainfall. Using radar and other forecasting tools, a seeder determines when a meteorological situation is ripe for seeding and proceeds to seed by an accepted method. The criteria for a suitable seeding situation vary, but regardless there are many situations that are unsuitable. The rainfall is recorded by some combination of radar detection and local raingauges. The operation continues until either adequate rain has fallen or the contract has expired.

On the basis of the rainfall data how should one attempt to answer the questions of interest? It seems clear that one must in some way obtain an estimate of the amount of rain that would have fallen naturally on the seeded occasions. Meteorological forecasting estimates are inadequate. The most common method is to find a group of similar occasions that can serve as "controls". If all such occasions were identical one would be done, as then any difference between rainfall on seeded and unseeded occasions could be attributed to seeding. However, a tremendous amount of variation exists among such occasions. It is because of this variation that the methodology of statistics is required. It is also because of this variation that the two questions cannot be answered with certainty: Only probabilistic statements about the existence and size of a seeding effect can be made. Before discussing these issues the definitions of comparative experimental design must be introduced.

To begin, the objective of a comparative experiment is to compare the performances of certain repeatable operations or processes when applied to a group of well defined objects. The repeatable operations or processes are called "treatments." In this case the treatments are seeding and no seeding (control). The objects are called "experimental units" or "EU's" for short. In this case there is a necessary arbitrariness to the definition of an experimental unit and this will be examined later in the paper. For simplicity, the definition of an EU will be the storm over a fixed target from the time of seeding until 12 hours later. The performances of the treatments are judged on the basis of some measurement that reflects the amount of rainfall. The choice of relevant measurement, again, is somewhat arbitrary but, whatever the measuring device or technique, it must be consistent. Here the measurement is assumed to be the average hourly rainfall in the target area for the time period.

Returning to the example, one is faced with choosing a sample of EU's to serve as the group receiving the no seed treatment. This poses serious problems some of which will be examined. Before considering these we will describe the weaknesses inherent in any nonrandomized comparative study. For the moment we assume we have a fair group of control observations.

In all likelihood the average hourly rainfall for the control group and the average hourly rainfall for the seeded group will be different. How does one judge whether the observed difference is real or simply a chance result due to the large variability among suitable seeding days? Resolution of this question requires making assumptions about the meteorological processes involved. These assumptions are collectively known as the model. Thus the validity of the model depends on the current knowledge of the physical processes. To date this knowledge is not adequate.

> "The complexity of cumulus processes and interactions is so great, however, that despite its admirable progress, modeling is still in its infancy. All models are, of necessity, so tenuously posed upon hierarchies of assumptions and over-simplifications that while their use in guiding and evaluating experiments is of great value, no model or models can substitute for or replace statistical randomization in the foreseeable future. Nor can they be used to replace measurements."
>
> (J. Simpson in Sax, et al., 1975)

The conclusion is that one cannot reasonably adopt a model that can be used to make inferences about the existence of an effect.

However, to consider the problem of choosing a sample of controls we will adopt the usual model for analyzing an operational program. This model is termed the "historical regression" method. An area, far enough removed from the target area so as not to be contaminated by seeding, is designated to be a comparison area. The area is chosen so there is as high a degree of agreement as possible between the amount of rain that usually falls in the target area and the corresponding rainfall in the comparison area. For the control sample one attempts to choose a group of similar suitable seeding days from historical meteorological data. Armed with the necessary assumptions one can estimate the relationship between the comparison area and the target area for the unseeded days. This estimated relationship is termed the "historical regression line" and is used to estimate the amount of rain that would have fallen naturally on the seeded occasions. The inference is that any observed differences are caused by seeding.

In any comparative experiment this final inference must be carefully questioned. Whether there was some factor other than seeding that distinguished the two groups and that could have caused the observed difference is always a real and important concern. Statistical research and speculation have identified several factors that could favor one particular group in this historical method.

A first problem is that measuring devices and methods would quite likely be different for any historical period chosen. It is easy to imagine situations where, because of different measuring techniques, one group would be favored (Brownlee, 1960).

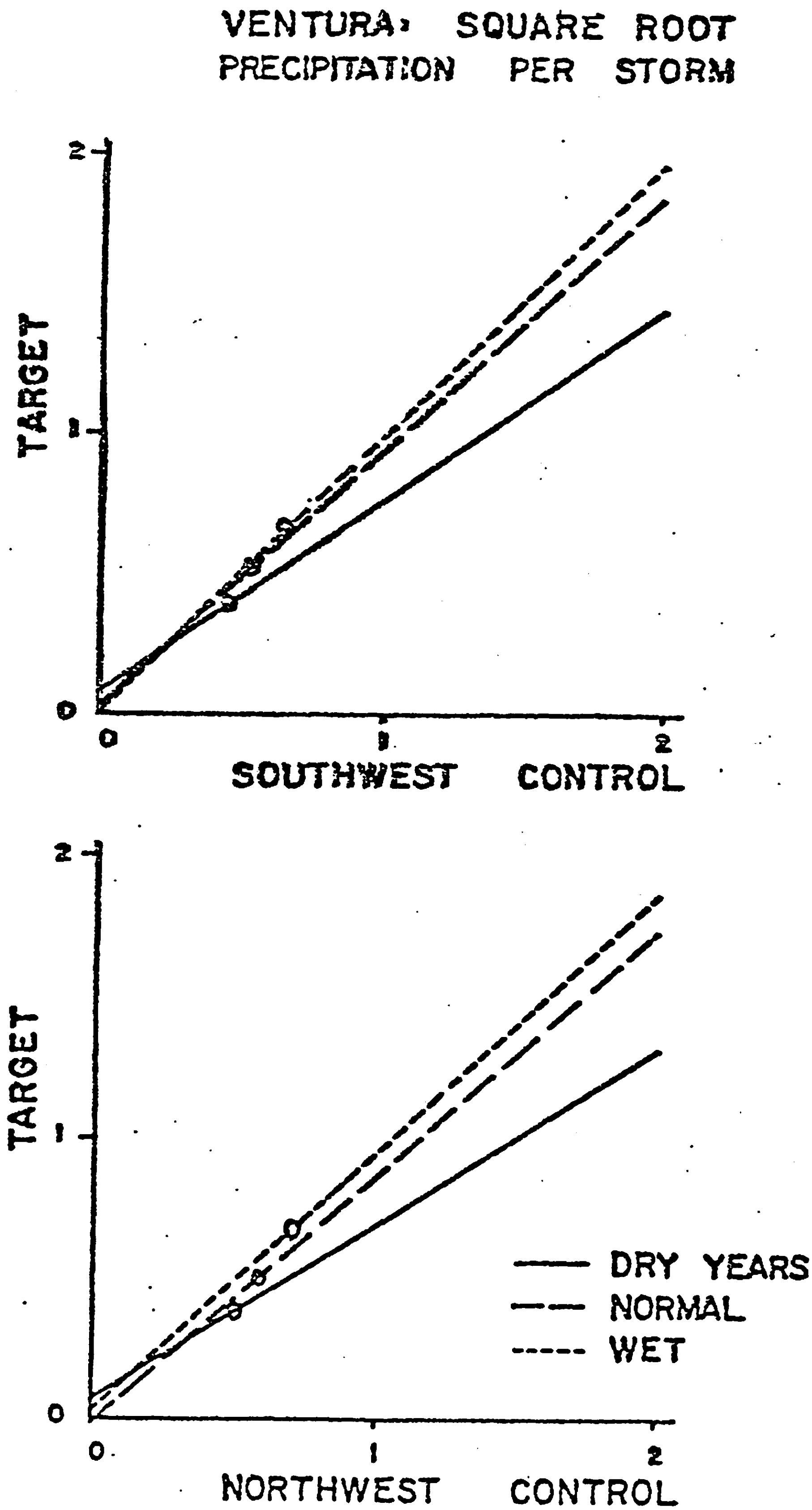
A more serious problem is that commercial operators could consciously or unconsciously choose suitable seeding days that more than naturally favor the target area over the comparison area. The historical control group is usually, and probably necessarily, chosen from all storm days which would reflect a natural relationship between the target and comparison areas. Hence the analysis could show a positive seeding effect that was due in fact to the selectivity (Brownlee, 1960). A rebuttal to this criticism is that commercial operators seed nearly all storms passing over the target area. A study of three large projects in California, however, showed that the operators seeded only about 60% of the days when substantial rain fell on the target. Furthermore, there is some evidence that the operators did indeed succeed in seeding on days that favored the target area (Neyman and Scott, 1961b).

A further deficiency of this method is that it is doubtful that the relationship between the target and comparison area remains constant over time. Brier and Enger show the relationship to be unstable for an Arizona project. Others have also presented evidence of such instability (Brier, 1974). Thus, any conclusion is highly dependent on the historical period selected for the controls (See Figure 1 for an example).

One final possible problem is that of optional stopping. If there is a sufficient amount of rain during the early part of the seeding period, the operator may quit seeding before the end of the contract. An analogy is one of quitting a poker game once one is ahead (Brier, 1974). Consequently, in the absence of any seeding effect, a certain percentage of the time the seeded group would have an excessively large estimate because of this subjective termination. Assuming use of the historical regression method T.E. Harris has shown that the bias in favor of the seeded days could be as much as 5% (NAS, 1966).

From the preceding one can fairly say that any analysis of the usual nonrandomized operation is questionable for two reasons: The first is that with the current state of meteorological science one cannot reasonably assume a model for the analysis. The other is that regardless of how the controls are chosen, there is a distinct possibility that some other factor

Figure 1



HISTORICAL REGRESSION LINES FOR A TARGET AREA IN VENTURA COUNTY, CALIFORNIA, ESTIMATED FOR TWO DIFFERENT COMPARISON (CONTROL) AREAS FROM DRY, NORMAL, AND WET HISTORICAL PERIODS (NEYMAN AND SCOTT, 1967d).

biases the results in favor of either the seeded group or the controls. Hence for a valid evaluation of a seeding program one must use a sound experimental procedure. Any such procedure necessarily employs randomization. The explanation of how randomization removes or alleviates the above problems is discussed in the next section.

2. The Benefits of Randomization

> "When a commercial cloud seeding operation is conducted without randomization, this must be of concern to the consumer who pays for the seeding rather than to outsiders. However, when a piece of research is financed from public funds and an expensive trial is performed without randomization, then this is the subject of legitimate concern to the public at large and, particularly, to the scholarly community."
> (Neyman and Scott, 1967d).

The above statement was made by Jerzy Neyman and Elizabeth Scott in a paper delivered at Skywater Conference II in 1967. These Berkeley statisticians have done much work in the weather modification field with Neyman's dating back as far as 1950. The position of this paper is that the State's desire to allow for adequate evaluation of operational cloud seeding programs places them in the role of the consumer in the quotation. Despite the cautions of the previous section and advice such as the above, there is still some skepticism over the need for randomization. It is the purpose of this section to explain the benefits of randomization.

Suppose for simplicity that over a contract period there are 30 days suitable for seeding. Now if one knew how much rain would naturally fall in these days, one could seed all 30 and record the average hourly rainfall. The difference between the observed seeded average and the known natural average could be said to be caused by seeding. Of course one does not know the natural rainfall beforehand, and as shown above the natural rainfall cannot reasonably be estimated from historical data. Thus, one is forced to leave some of the 30 days unseeded to serve as a control group. If all 30 days are completely identical, then one would have to leave only one day unseeded since this would give the true average natural rainfall for all 30. Again the question of the effect caused by seeding would be answered exactly. However, as noted, there is huge variability among suitable seeding days so this approach will not lead to exact answers.

Hence one is faced with essentially the same problems as in an evaluation of the usual operational program: First is how to select the control group to minimize the possibility of bias. Second is how to handle the variability among the suitable seeding days.

The basic strategy is as follows: One selects a subgroup of the 30 suitable days and leaves them unseeded. By some model these control observations are used to estimate the natural rainfall for all 30 days. Similarly one obtains from the seeded days an estimate of the seeded rainfall for all days. Subsequently, probabilistic statements about the true difference between the seeded average rainfall and the natural average rainfall over all 30 days can be made. Given these probabilistic statements, the method of their computation, and the knowledge of the choice of the control group, one decides how much faith to place in an inference about seeding effect.

We first consider what appears to be an obvious procedure. One splits the entire group into two "balanced" groups assigning one to be seeded and one to be natural. The balancing is done on factors considered by meteorologists to be relevant to rainfall and is done to produce as fair a division as possible. This is quite hard to accomplish in cloud seeding since one does not know the characteristics of a suitable day until that day arrives and hence the balancing plan cannot be completely determined beforehand. For the sake of illustration, suppose this balanced division can be made. Presumably the seeded days will not be favored and one can simply compare the average rainfall for seeded days and control days. However, even for days agreeing on levels of the balancing factors there will still be considerable variation. Thus we need some way of estimating and utilizing this variability in the analysis. Unfortunately to handle this variability one must again adopt a model which, as was noted, is currently extremely difficult. This drawback is sufficient to eliminate any such balanced designs.

It should be mentioned that there is another problem with balanced designs. It is quite possible that balancing will produce a more equitable division of the suitable days. But it is also a possibility that, while balancing on known factors, one is favoring one group with respect to a relevant unknown factor.

This leads one to divide the group by some other means. In retrospect it seems the next logical method is to select the seeded days and control days by some random process. Sir Ronald Fisher developed this method in the 1920's and his book Design of Experiments is now a classic in the field. His idea was not completely accepted at first, and there was much debate in the 1930's over the merits of randomized designs vs. balanced or systematic designs. As Fisher wrote though, it is possible to incorporate some elements of balancing in a randomized design. This is known as blocking and will be explained in the design section.

A word of warning is appropriate. The actual random division into two groups must be done by some physical device that is known to behave in a random fashion. Coin flipping, card drawing, or a table of random numbers prepared by such a physical device could be used. Studies have shown that humans cannot make random choices simply by will. For a discussion of performing a random selection, see Experimental Designs by Cochran and Cox.

The reason randomization solves or nearly solves the problems inherent in any analysis of a usual operational program or a systematic design is that the randomization process itself provides the model for the analysis. Nearly all assumptions composing the model depend solely on the properties of the randomization process which presumably have been validated. The few assumptions not warranted by the randomization process (and also not always needed) are inescapable in any other usable model as well. Thus randomization can, as adequately as possible, provide a model when the state of knowledge cannot. Consequently, randomization is an experimental tool necessary in fields of experimentation where reasonably validated models do not exist.

However, any particular randomized assignment may excessively favor one treatment over another and this must be recognized in extending the statistical analysis to an inference of a cause-effect relationship. This possibility is currently what makes the results of Project Whitetop controversial. But the randomization process also guarantees that this happens only a small percentage of the time.

A randomized experiment puts any cause-effect inference on much firmer ground. In fact some would say that only from a randomized experiment can such inference be made.

> "But as soon as one considers a physical area, such as weather modification, in which 'identical units' are not available, the role of randomization is considered crucial, and no nonrandomized experiment is considered to have any inferential value."
> (Kempthorne, 1977)

One concluding point must be emphasized. Any inferences from an experiment are made only in regards to the EU's and conditions of the experiment, unless the units are randomly sampled from some larger population of units. Any extension of the inferences beyond the experiment is what Kempthorne terms a "nonstatistical inference" and is based on the assumptions that conditions and units outside the experiment will be similar to those in the experiment. In weather modification this is certainly necessary since one cannot sample days from time beyond the experiment for inclusion in the experiment. One must be aware of these further assumptions in the application of results from any experiment.

A more technical discussion of how hypothesis testing and estimation for the example can be based on the randomization process is given in Appendix B. A particular example of how a randomization scheme can be implemented is in the following section on Project Whitetop.

3. <u>Project Whitetop</u>

There are several reasons why a discussion of Project Whitetop in Missouri is pertinent. For one, the design and results of the program highlight the issues currently faced when designing a cloud seeding experiment. The program itself was well conceived and implemented, and its basic structure will be recommended in the following section. For another, of all large cloud seeding experiments, it is probably the most relevant to Minnesota's meteorological and topographical environment. Indeed, one reason Missouri was chosen was so, ". . .any findings about cloud seeding would be immediately applicable to a wide area of the agricultural heart-land of midwest United States." (Braham, 1966, p. 3). Finally, if any conclusion can be drawn from the results it is that seeding <u>decreased</u> rainfall and over a much wider area than expected.

Project Whitetop was conducted by a University of Chicago team headed by Professor Roscoe R. Braham, Jr., of the Department of Geophysical Sciences. Seeding was done in the area of West Plains, Missouri, over the summer months of the five years, 1960-1964. The project had two objectives: "(a) to further elucidate the physics of rain production in summer convective clouds of the mid-continent, and (b) to investigate, by means of a randomized cloud seeding project, the effect of silver iodide in altering either the physical character of the clouds or the rainfall from them." (Braham, 1966, p. 1). This section explains how the second objective was reached.

The first treatment, seeding, was done along a 30-mile line roughly perpendicular to the prevailing winds and upwind of the target area. It was done by three airplanes operating individually in three contiguous

10-mile strips. The seeding agent was silver iodide and it was dispersed by acetone burners, one under each wing. Seeding was continuous for six hours beginning in mid-morning. This timing was used because earlier study had shown that summertime cumulus showers most often occur in the middle or late afternoon. Morning showers were usually remnants of nighttime thunderstorm activity. The seeding rate was much higher than had been used in previous experiments (Braham, 1977). The second treatment was no seeding or control with all other activities done as usual.

The definition of the experimental unit was complex and in some respects was allowed to vary. Suitable seeding days were determined by criteria that evolved from a study of the meteorological history of the area. The criteria were mainly early morning precipitable water at Little Rock, Arkansas and Columbia, Missouri and various wind conditions. The actual target area was determined by the radar unit used for measurements. The unit was placed outside West Plains and could observe a circle of radius 60 miles. Once seeding or no seeding was defined as begun, a "plume" was delineated. The plume was estimated from balloon wind measurements taken every two hours. The original plumes were based on winds from flight level, usually about 4000 ft. msl., up to 14,000 ft. msl. It was believed that these plumes would contain all areas possibly affected by silver iodide as well as some not affected. Hence the area inside the target circle and outside the plume was presumably not affected and could possibly be used as, what is termed, a covariate in hopes of increasing precision. These plumes were later named the "Chicago plumes." A smaller plume with most likely a heavier concentration of silver iodide was later defined solely by wind measurements at flight level. The area outside these plumes was considered contaminated and could not be used as a covariate. These plumes were called the "Missouri plumes." The target area was measured hourly from the onset of treatment until midnight or until the plume had left the target circle, whichever occurred first. So an experimental unit was essentially the life, inside the target, of an air mass plume originating from a 30-mile line.

The principal measurements for evaluation were radar echoes and raingauge readings. From a meteorological view radar is the principal measuring tool because it gives a, "nearly simultaneous view of the three-dimensional structure of every rain cell it examines." (Braham, 1966, p. 33). However, radar is not a good estimator of intensity of rainfall; so for this purpose a raingauge network was used.

The raingauge network consisted of 36-49 gauges, 28 of which were the normal U.S. Weather Bureau gauges. There was roughly one raingauge for every 250-300 square miles and this was admitted to be perhaps insufficiently dense (Decker and Schickedanz, 1967). Again, readings were taken hourly. This was necessary for the plume analysis and also proved quite productive in later non-plume analyses. Besides the balloon measurements, other meteorological data were gathered with an observation plane. These included such variables as different temperatures, water content measurements, and particle size distributions. Some of these were later used as covariates. Cloud stereo photographs were also taken.

The actual structure of the randomized design is called an unrestricted 50/50 randomized design. In each year a sequence of instructions, one per summer day, was prepared. Each instruction was either seed or no seed, picked from a random number table so that the probability of seeding or not seeding on any given day was ½. Also the selection for any one day was independent of that for any other day. These instructions were sealed in envelopes with the appropriate date labels. Only after a day had been declared suitable for inclusion in the experiment was the instruction envelope opened. Envelopes for unsuitable days were returned unopened. Any subjective part of data determination, such as reading radar photographs, was randomly coded so that the reader had no knowledge of whether or not a day was seeded (Braham, 1977).

The results of Project Whitetop have proved to be controversial. Over the five summers 198 days were included in the experiment. By the random selections 102 were seeded and 96 were not. The original raingauge analysis was done using the average within plume hourly rainfall. The analysis of the Chicago plume without covariate gave an estimated <u>negative</u> effect of seeding although it was not particularly strong. The analysis of the Missouri plume, however, did give a significant <u>negative</u> estimate of the seeding effect (Decker and Schickedanz, 1967). Subsequent analysis on the Chicago plume using the covariate gave the estimated overall effect of seeding as negative for both echo cover and raingauge measurements. There was weak support for the raingauge results and moderate support for the echo cover (Braham, 1977). Analyzing various subsets of the data and incorporating these with meteorological measurements lead the Whitetop scientists to the tentative belief that the negative effect had been caused by over-seeding of the larger clouds. The conclusion was that blanket seeding of whole fields of clouds could be counterproductive (Braham, 1977).

Berkeley statisticians analyzed the data in a somewhat different manner. They looked at 24 hour raingauge data in six concentric rings each of diameter 30 miles. Hence this involved using only Weather Bureau gauges for the rings beyond 60 miles from West Plains. Their analysis showed the negative effect extending to all rings with the effect decreasing with distance from the center. It also showed that there was a 3-4 hour lag time from the outset of seeding until the effect was observed in the target circle. Similar results held for the outer rings (Lovasich, et al., 1969b; Scott, 1973). Further analysis by the Berkeley group, however, indicated that the control days were inordinately favored with wet weather, putting any final inferences in doubt (Lovasich, et al., 1971b). Braham does not agree with the qualifications placed on the conclusions by the Berkeley group (Braham, 1977).

Controversy notwithstanding, there are some indications that must be heeded by other experimenters. The possibility of extended area effects cannot be discounted and such things must be accounted for in the design and measurements. Effects quite probably have a lag time so measurements should be taken over a longer period of time and should be taken at hourly intervals. There should perhaps also be some "rest" period after a seeded unit. Finally, there is sufficient reason to believe that seeding effects can be negative.

Appendic C gives a table that summarizes the results of randomized experiments performed to examine the effects of cloud seeding on rainfall. The seeding methods, environmental conditions, and length of the experiments vary considerably; but nevertheless the table indicates that inconsistent or unexpected results are by no means confined to Project Whitetop.

4. Designs for Operational or Experimental Cloud Seeding Programs

4.1 General Considerations

Before discussing the issues of treatments, experimental units, measurements and the rest, it is wise to note an additional goal of a comparative experiment. Recall that two of the primary goals were to test for the existence of an effect and to estimate its size. These goals could be termed ones of confirmation and the establishment of hard evidence (Kempthorne, 1977). For the evaluation of an operational project these are perhaps the only desired goals. The researcher though usually wishes to extract more information from the experimental program. In particular the researcher attempts to determine whether relevant subsets of the experimental units show more pronounced effects than the experiment as a whole. This is termed post hoc stratification or partitioning and is done on the basis of meteorological variables or other objective criteria. For example, seeding experiments have been partitioned on the basis of cloud top temperature, wind direction, and storm type (NAS, 1973). The results of such post hoc stratification should in most, if not all, cases be considered only as indicators of effects and not as hard evidence. The indicated effects must be verified by future experiments or to some extent by reanalysis of valid old experiments. Some of the following recommendations will be based on partitioning possibilities and may not be relevant to an operational program.

Treatments

As mentioned in section 2 the treatments in a cloud seeding experiment are seeding and no seeding. For logical reasons these treatments must be consistently applied. For no seeding this is not a problem, but it is not unusual for the seeding method to be altered in an operational project.

A radical example would be if an operation changed nucleating agents from silver iodide to dry ice. One would have to consider the silver iodide seeding as one treatment and dry ice seeding as another, giving essentially two smaller experiments. A minor adjustment in treatment occurred in Project Whitetop when the start of seeding for a suitable day was moved from 11 a.m. to 10 a.m. (Braham, 1966). This was not considered to be an important change and the results of the experiment were based on only one seeding treatment. So the judgment as to the seriousness of a change in method is to some degree subjective. One can always perform the analysis using more than one seeding treatment looking for an indication of a difference in seeding effects. This was done in the Florida Area Cumulus Experiment (FACE) when the type of the silver iodide pyrotechnic flares was changed (Woodley, Jordan, et al., 1977). The point to be remembered is that any significant change in treatment method must be recognized in the analysis. The actual seeding method to be used depends of course on the beliefs of the operator or experimenter.

Experimental Units

By some criteria the cloud seeder determines whether conditions are ripe for seeding. Once conditions are deemed suitable, the problem of defining an experimental unit involves specifying the area where measurements are to be taken and the time period.

In an operational program the criteria are chosen by the commercial seeder. This subjective approach has also been the rule for research experiments. The experimenters attempt to show they can increase rainfall under certain conditions and thus restrict their EU's to those conditions. However, rarely do they obtain convincing results and indeed sometimes find the opposite results. For this reason Professor Neyman suggests that a research experiment use all but the clearly unsuitable days for EU's. This requires more effort and money but does not increase the time needed for the experiment. Also one can legitimately analyze separately those EU's associated with the preconceived optimal criteria, achieving the original confirmatory goal of the experiment. Neyman's suggestion retains the objectives of verification and gathering of hard evidence but considerably increases the range for an exploratory analysis. Given the incomplete state of meteorological knowledge, he feels this is the most efficient use of time and money (Neyman, 1967).

The physical area for measurement can either be a fixed piece of land or be allowed to change or "float." The floating target is defined by the spread of the seeding agent and is determined as in Project Whitetop by wind measurements or as in FACE by radar echoes. The advantage to a floating target is that presumably the target would be contained in the area of maximum effect and thus the chances of confirming a seeding effect would be enhanced; that is, there would be no unaffected areas in the target that could dilute the results. The advantages to a fixed target are primarily ones of convenience and preciseness of definition. FACE used a fixed target and a floating target contained in the fixed target and subsequently both sets of measurements were analyzed. The Berkeley group primarily used a fixed target in their reanalysis of Project Whitetop. In the analysis of extended area effects it is usually necessary to use a fixed target since most radar are not sufficiently powerful to define a floating target outside the immediate target area. In an operational program the target is fixed, being the area listed in the seeder's contract. To answer the question of whether the seeder increased rainfall for the contract area one must analyze the fixed target measurements. However, it would also behoove a seeder to define a floating target within the contract area. This would require more effort but perhaps would increase the possibility of demonstrating some seeding effect for the contracted area. For a discussion of defining floating targets see the reports on Project Whitetop or FACE.

Similarly, the time period for measurements can be fixed or allowed to vary. When a time period is allowed to vary it is defined by a meteorological criterion such as the end of a storm. Also, the starting point of the time period can be fixed or allowed to vary. Again the variable starting point is determined by an objective meteorological condition. There appear to be two main advantages to a variable time interval. A time interval defined by meteorological conditions may allow for more relevant stratification in an exploratory analysis (Moran, 1970). Furthermore, if the time interval is based on the length of a storm and if seeding affects the length of a storm, then the seeding effect will be more readily discovered with that time interval. The main problem with a variable time interval is determining a proper definition. This plus convenience considerations has caused most experimenters to use a fixed time interval. For the same reasons a fixed starting time is usually used, although the Atmospherics Incorporated 1976 operational program in southwest Minnesota used a variable starting time (Henderson, 1977).

The length of fixed time intervals have ranged from 4 hours to 2 weeks (where seeding was done throughout the 2 weeks, Smith, 1967). The rationale for choosing a length is that it should be sufficiently long to allow the full effect to be manifested and yet not so long as to excessively reduce the total number of EU's possible for a project or mask any effect by the addition of natural variation. On the basis of the Berkeley group's analyses of Project Whitetop, among others, it appears that a 24 hour interval with at least 18 hours after the cessation of seeding is adequate. This will contain most of the effect and yet allow one EU per day provided seeding doesn't continue for more than 6 hours. More will be said on this in the discussion of persistence effects.

In short, the definition of an experimental unit is quite complex and usually arbitrary. Further research into these problems should improve the efficiency of cloud seeding designs. For the operational project most decisions are left to the discretion of the operator with the exception of the target being fixed. It is recommended though that it is to the seeder's advantage to have the measurement intervals at least 24 hours in length.

Measurements

Rainfall

As mentioned in the description of Project Whitetop radar yields a valuable three-dimensional picture of cloud systems. It can also determine the spatial variability of rainfall and can provide a rough estimate of the intensity of rainfall. By itself, though, radar is too inaccurate for measuring the magnitude of rainfall in a cloud seeding experiment. Some raingauges have to be used for measuring magnitude. They can either be used for corrections of radar estimates or as the sole measuring instruments. It was found that when used for radar correction a less dense raingauge network was needed for the same accuracy (Woodley, et al., 1975). This method was employed in FACE. Most experiments have used strictly raingauge data or, like Whitetop, have analyzed gauge and radar data separately.

One desirable, though not essential, quality of a raingauge network is that it can be read hourly. This allows for the examination of the timing of an effect. The principal problem in the design of a network is the question of density. It is intuitive that the more accurate the measurement of areal rainfall the easier it will be to detect a seeding effect. On the other hand there is a point of diminishing returns in increasing the density. To adequately determine a sufficient density one must know the spatial variability in rainfall, the storm to storm variability, and the effect of seeding on rainfall. Some attempts at determining the sampling error for a raingauge system in Illinois were made by F.A. Huff. Although he used regression methods based on some simplifying assumptions, his 1970 paper could be used for a rough estimate of sampling error in a Minnesota project. In general Huff found that sampling error increased for increasing areal mean precipitation and decreased for increasing gauge density and storm duration. Most experiments appear to use as many raingauges as economics and terrain will allow. Rules of thumb abound. For example, A.S. Dennis feels that in the usual experiment a raingauge every 5 miles is adequate (Dennis, 1967). It would also be advisable to have a reasonably dense system outside the target for the measurement of extended area effects. This is usually not feasible, however, and one must rely on existing U.S. Weather Bureau gauges.

Hail

With obvious modifications such as in seeding criteria, the basic designs and discussion of this paper apply to hail reduction projects. The primary difference is one of measurement. Stanley Changnon's 1969 review article discusses the advantages and disadvantages of various hail measuring methods. For random daily seeding in the midwest, Changnon decided the only suitable measuring device was a recording hailgauge. Such gauges have been developed but are unproven. Furthermore, the large spatial variability of hail requires a very dense network of gauges. Initial estimates are that an adequate network needs 1 gauge per 2 square miles (Changnon, 1968). Crop insurance records, radar, and hailpads are considered to be only partially suitable measuring instruments.

Covariates

Covariates, also known as predictor or concomitant variables, are commonly used in research work to improve precision, i.e. reduce variability. Although their validity is not based on randomization, some work has indicated that for a large number of EU's, as in a cloud seeding experiment, this method is probably not too biased as an approximation to an unbiased randomization procedure which is difficult to perform (Cox, 1956). In any case, covariate analysis is an accepted statistical procedure, particularly useful in weather modification where there is a need to reduce variability.

A covariate is a variable that is measured on each EU and is assumed to be unaffected by any treatments. If the variable is measured before application of the treatment this is certainly true. For a covariate to be useful it must be related to the measured response as well as independent of the treatments. In that case covariate analysis linearly adjusts for the effect produced by the covariate. This reduces the variability and increases the sensitivity of the experiment for detecting a treatment effect.

Some covariates that have been used are rainfall in a nearby comparison area, amount of rain in the target area the hour before seeding, per cent of radar echo coverage in the target area before seeding, and certain seeding suitability variables. However, the strong possibility of extended area effects means that rainfall in a comparison area is quite possibly affected by seeding and thus should not be used as a covariate.

The need for finding effective covariates both for increasing the sensitivity of experiments and for understanding the meteorological mechanisms is great. In fact Elizabeth Scott feels, "Improvement in design and analysis of cloud seeding experiments seems to reside in improving prediction." (Scott, 1973). Currently there appear to be no outstanding covariates. One could use some of the above most of which were used in FACE (Woodley, Simpson, et al., 1977). Any mildly effective covariate improves the chances of demonstrating a seeding effect so it behooves the operator to record any possibly relevant covariates for use in the analysis.

Extended Area Effects

Extended area effects are seeding effects that occur outside the target area, an example being downwind effects. The Berkeley group's analyses of Project Whitetop, Grossversuch III in Switzerland, and the Arizona experiments give strong indications of extended area effects in all directions, particularly downwind. These effects can extend as far as 200 miles and are in the same direction (positive or negative) as the effect in the target area (Neyman, 1975). In any cloud seeding experiment, the design must recognize the possible existence of such effects and the evaluation must include them. As mentioned above the measurement of rainfall in the extended areas will usually be from Weather Bureau gauges although a denser network is preferred. It should also be noted that it appears seeding effects reach over a longer time span than originally expected. Consequently measuring extended area effects should correspondingly continue beyond the interval set for the target area. The actual analysis of extended area effects can be done in the manner of the Berkeley group's analysis of Project Whitetop (see Lovasich, et al., 1969b, 1971a, 1971b; Neyman, Scott, and Smith, 1969).

Persistence Effects

Persistence effects are the aforementioned effects that extend for longer than expected time periods. The Australia experimenters were perhaps the first to hypothesize the existence of such effects (Bowen, 1966). In an attempt to avoid long-term persistence effects and to investigate their existence a Tasmania experiment used only experimental units from alternate years while continuing to carefully record rainfall in the "rest" years (Smith, et al., 1971). The Berkeley group's work on Grossversuch III, however, indicates this long range persistence effect is not a particular problem (Neyman and Scott, 1967d). Their work does indicate though that a possibly large short term effect could extend into the day following seeding (Neyman, 1975). For this reason it is recommended that a day immediately following a seeding day not be declared an EU regardless of its suitability.

4.2 Designs

A Note on Analysis

As explained earlier the logical basis for any analysis is the randomization scheme in the design. The exact randomization methods similar to those illustrated in Appendix B can be used, but for some aspects of this analysis expensive and arduous computer work is required. Often, approximations to the randomization method are adequate. In particular, an analysis using covariates requires linear model theory. For further explanation see Design and Analysis of Experiments by Oscar Kempthorne.

In any cloud seeding experiment there are usually many EU's for which no rain falls. This situation can be handled by doing two analyses, one on all EU's and one only for the EU's that received measurable rainfall. For an example of this see Neyman, Scott, and Wells' article "Statistics in Meteorology." On the other hand, it must be recognized that seeding may cause naturally rainy days not to rain and vice versa. Thus to answer the question of whether seeding increased or decreased rainfall over the experiment one must include all EU's in the analysis.

Designs with Unrestricted Randomization

In this design there is one target area. Once a situation is determined to be suitable as an experimental unit a random decision is either made or, having been made earlier, is revealed. The random decision is usually generated so that each treatment has an equal probability, i.e. a 50/50 design. The design need not be 50/50, but it is advisable since this both simplifies the analysis and most likely maximizes the chances of detecting a seeding effect. Each randomized decision is probabilistically independent of any group of others. This design was used in Project Whitetop.

There are several advantages to this design. Since it only has one target area, there is no problem in estimating extended area effects. Since each decision is independent of any group of others, there is no problem of conscious or subconscious selection bias, a matter which will be discussed below. The simple randomization structure makes all computations easy if perhaps somewhat time consuming. The structure also allows for easy analysis of any partition of the experimental units. The lack of restrictions in the randomization gives as precise an estimate of the variability as possible in an experiment of the same size.

The disadvantages of this design are few. Since the randomization is completely unrestricted there is a possibility of an inordinate number of EU's receiving one treatment. This would pose a problem in a small experiment. Also the design makes no provision with the exception of using covariates for reducing the variability in the experiment.

For a large experiment such as Whitetop this design is recommended with the reminder that relevant covariates may be very beneficial.

A Slight Modification

For a smaller experiment such as an operational program a minor modification can be made in the above design. One can guarantee that in the randomization process each successive group of, for example 10, EU's has half seeded and half not seeded. This means that regardless of when the program stops no treatment will have more than 5 observations over the other. This type of modification was used in Grossversuch III and FACE.

This design has most of the advantages of the unrestricted design. The computations are slightly more involved and any partitioning analysis is not quite so easy. Also, the estimate of variability is not as precise. However, the design itself will remove some variability from the experiment if the variation among the groups of EU's is greater than the variation within the groups. This is the principle of blocking and will be discussed in the next design.

The main disadvantage of this design is the problem of selection bias. Selection bias can occur when the experimenter has a better than even guess as to what treatment will be applied to the next experimental unit. The experimenter can then consciously or unconsciously pick the next EU to favor one treatment's results. For example, suppose in the above illustration that after 8 EU's 5 had been seeded and 3 had not. The experimenter would know that the next 2 EU's would not be seeded and consequently could pick poor rain days for the next 2 EU's biasing the results in favor of seeding. There are two ways this problem can be overcome. For one the suitability decision can be made by a disinterested third party or by some completely objective criteria. Also the results of any decisions to seed or not seed can be withheld from the experimenter until the end of the experiment (Stigler, 1971). It seems only the first method is feasible in an operational program.

If selection bias can be eliminated this design is particularly recommended for an operational seeding project. Guarding against selection bias though would probably require a state inspector for all determinations of EU's.

Randomized Block Design

This design again uses one target area. It attempts to remove a large part of the variability of response in the experiment by the principle of blocking. If one can choose EU's in groups of two, known as blocks, so that the natural variation among the blocks is greater than that within the blocks then the precision of the comparison of seeding to no seeding can be improved. This is done by assigning one treatment to one EU in a block and the other treatment to the remaining EU. These assignments are done randomly with each treatment having equal probability of being assigned to each EU, and the assignments for any one block are independent of those for any group of other blocks. The precision of the comparison is increased because the comparison is based on the average of the differences of the treatment responses within blocks. The main problem in applying this design to cloud seeding experiments is finding an adequate variable on which to base the construction of effective blocks. This is the same problem as finding an effective covariate. One method that has been used in the Arizona experiments is blocking in time (Neyman, 1975). That is, if one EU was seeded the next was not and vice versa, consequently forming the blocks. It does not appear that this particular blocking scheme was especially effective in reducing variability.

The advantages of this design are similar to those of the modified design above. The major disadvantage of this design is again the possibility of selection bias. However, this can be prevented as described above. If an effective means of block construction can be determined, this design is recommended.

Crossover Design

This once popular design is currently in disfavor but will be included here for completeness. Two target areas are used being far enough apart to preclude contamination and yet close enough to have similar rain measurements. For a suitable EU one area is randomly assigned with equal probability to be seeded while the other is left unseeded. The assignments are independent from one occasion to another. In this way one obtains twice as many observations in the same amount of time as the above designs. The fact that the two target areas have, on the average, similar rain measurements can lead to improved precision in the same fashion as blocking. Thus, we obtain reduced variability and double the number of observations. If the assumption of no contamination is met this design is certainly the most efficient design presented here. The design originated in Australia and has been used in most of their experiments. It has also been used in the Israel, Quebec, and South Dakota experiments. It is not usually applicable to operational programs since it requires two target areas.

The design has two fatal flaws though. Extended area effects cannot be estimated since the extended area for a no seed observation overlaps the extended area for the seeded observation occuring at the same time. More importantly the Berkeley group's analyses have cast serious doubt on the assumption one target area is not contaminated by the other. This design is for the moment not recommended for any seeding program.

5. Summary and Recommendations

This paper has attempted three tasks: First, it tried to demonstrate that any evaluation, no matter how careful, of the usual nondesigned operational program is necessarily based on debatable assumptions and consequently is open to honest criticisms. Furthermore, while such a careful analysis may be taken as a possible indication of a seeding effect, it cannot produce hard evidence of a seeding cause-effect relationship. Second, it attempted to show that in order to validly determine such a relationship one must by some random scheme let some of the suitable seeding days go unseeded to serve as a control group. Finally, it presented several adequate designs for operation based on the results of research experiments in the field.

There is one additional point, however, that must be recognized. Using a statistically sound design assures a valid analysis, but does not guarantee a high chance of detecting a seeding effect when in fact such an effect exists. This is simply because the natural variability among the experimental units is large relative to the size of the probable increase (10 - 20%) in rainfall. In other words it may happen that in a large percentage of experiments the rainfall variability will mask a 10 - 20% increase in rainfall caused by seeding. Thus, if one plans to implement such an experiment, it is advisable that at least a rough estimate of the chances of detecting the relevant effect be obtained. Unfortunately to obtain the true chance requires knowing the nature of the physical processes, which is not currently possible in cloud seeding. However, with simplifying assumptions and models derived from historical data one can hopefully get a crude approximation. Little work on this problem has been done, but the Berkeley Statistical Laboratory did derive approximations for one case (Scott, 1973): Suppose cloud seeding actually produces a 20% increase in rainfall on naturally rainy days. Suppose further that the program runs long enough to have 100 suitable seeding days, which are used in a 50/50 unrestricted design. (It appears it would take roughly 3 full summers of seeding to obtain these 100 suitable days.) Then the chance of reasonably concluding that seeding had an effect is approximately 20%. If the seeding increase is only 10%, the chance of reasonably detecting it is only approximately 15%. These chances are unacceptably low. Considering the expense of such an experiment, the advice is that the experiment is most likely a waste of time and money and should not be conducted.

There are ways of improving the chances of detecting effects. With knowledge of the physical processes one can choose suitable days that have less natural variability and hence would tend less to mask seeding effects. Also with this meteorological knowledge one can perhaps correct for the large variability using covariates or predictor variables. As noted several times earlier this knowledge is not now available. Another method of improvement is by discovering a more efficient design. However, with the problems of extended area and persistence effects the current opinion is that the designs given in this paper are as efficient as possible. Consequently the only other way of improving the chances of detecting effects is by increasing the number of experimental units. In most operational programs the option of increasing the length of the program would not be available.

One other possible way of increasing the number of experimental units is by combining the results of separate projects. Certain minimal conditions must hold, though, for this to be valid. For one the actual seeding methods must be identical, perhaps with the exception of minor changes. The definition of experimental units and the measurement methods must also be the same. The geographical area or the date of the projects do not, however, have to be the same. But how often these conditions can be met in practice is unknown. It is unwise to rely on the possibility of combining experiments to provide an adequately large sample without forced standardization.

The results of this research have led to several conclusions and recommendations. In particular, although a very careful analysis of a well recorded nonrandomized operational project may lead to an indication of a seeding effect, it could not be construed as hard evidence of a cause-effect seeding relationship. Any general policy of the State to analyze such projects would be a waste of time and money.

If the State does want to allow for a valid quantitative evaluation of a project, some suitable seeding days must be left unseeded by a random decision process. With the exception of the crossover design, all designs described above are adequate. In implementing one of these designs the following recommendations should be heeded:

1) A full-time inspector is needed to insure unbiased selection of suitable seeding days, correct randomization, and consistent recording of measurements.

2) The target area should be removed from other project target areas by approximately 200 miles to minimize the possibility of contamination. If this cannot be done, all possible contaminating projects must be combined into one large project or else the evaluation of all projects should be abandoned.

3) The day after a seeded day should not be an experimental unit regardless of its seeding suitability. This reduces the possibility of contamination over time.

4) A thorough and correct analysis of the results requires a level of statistical knowledge at least equivalent to a master's degree. Considerable computer time would also be needed.

Furthermore, to have any reasonable chance of detecting an actual 10-20% increase in rainfall produced by seeding currently requires well over 100 suitable seeding days, perhaps upwards of 200. If such a sample size is not expected from a considered project, either alone or in valid combination with similar projects, then insuring for a correct evaluation with an adequate design is also probably a waste.

In summary, a carefully planned and executed randomized design is necessary to a valid statistical evaluation of the effect of a cloud seeding project. If for whatever reason this is not done, no attempt at a quantitative analysis of the project should be made.

Appendix A: Glossary

Comparative Experiment - an experiment performed to compare the performances of two or more treatments on a group of experimental units. In a cloud seeding experiment the treatments are seeding and no seeding (control).

Covariate, also Concomitant Variable or Predictor Variable - a variable measured for each experimental unit that is assumed to be related to the response (rainfall) but not affected by any treatment.

Experimental Design - the instructions for the carrying out of a comparative experiment. This includes the definition of the experimental unit, the choice of experimental units, the choice of treatments, the choice and measurement of covariates and responses, the method of randomization, and the actual randomized assignment of treatments to experimental units.

Experimental Unit (EU) - the object to which a treatment is to be applied and upon which the measurement is to be taken. In a cloud seeding experiment it is usually the combination of a suitable seeding day, a target area, and a time interval.

Extended Area Effects - seeding effects that extend beyond the target area. An example is effects downwind of the target.

Historical Regression Method - method of obtaining an estimate of natural rainfall in a target area using the unseeded and assumed to be uncontaminated rainfall in a comparison area.

Operational Program - a program conducted for the purpose of implementing existing technology. It is usually not designed to allow for valid comparison of different treatments, for example seeding and no seeding.

Persistence Effects - seeding effects that extend beyond the expected time interval, whether short term or long term.

Randomization - the method of assigning treatments to experimental units by a random process whose properties have been confirmed.

Seeding Suitability Criteria or Suitability Criteria - the criteria that determine when a situation is amenable to seeding for a particular experiment.

Treatment - a repeatable stimulus or procedure that is applied to an experimental unit.

Appendix B: Hypothesis Testing and Estimation in a Simple Randomized Experiment

It will now be shown how in the simplest experiment the randomization probability distribution is used to answer the questions of hypothesis testing and estimation. First, some notation must be introduced.

Suppose the natural rainfall for 30 suitable days can be listed as:

$$X_1, X_2, X_3, \ldots, X_{30};$$

and the seeded rainfall can be listed as:

$$Y_1, Y_2, Y_3, \ldots, Y_{30}.$$

Although equal groups are not necessary, the assumption here is that 15 of the days are drawn at random from a deck of 30 numbered cards. These days are left unseeded and the remaining 15 are seeded. The following development is along the lines of Oscar Kempthorne's 1977 paper, "Why Randomize?".

Hypothesis Testing

The first thing one might want to do is test the hypothesis that there is absolutely no effect from seeding. This is equivalent to saying $X_1 = Y_1$, $X_2 = Y_2$, and so on. If that is indeed the case, the only reason the average seeded rainfall differs from the averaged natural rainfall is that the randomization happened to select two groups that differed. That is, the observed difference is caused entirely by the variation among the 30 days. Thus, for the sake of the test, one assumes that the above specific hypothesis, known as the null hypothesis, is true. Consequently all 30 observations are interpreted to be natural rainfall, i.e. X_1, X_2, $\ldots$, X_{30}. Using a computer one can then calculate the difference between the average rainfall for seeded days and the average rainfall for unseeded days for all possible random assignments. Since one knows each random assignment was equally likely, one knows the probability distribution of the difference in the averages assuming the null hypothesis is true. In a strict hypothesis test one has decided beforehand to reject the null hypothesis for outcomes whose probability totals α % under the null hypothesis. One usually chooses the upper and lower $\alpha/2$ % of the outcomes, since in the presence of negative or positive seeding effect one would most likely expect to see the smallest or largest values. So as constructed, if there is no seeding effect, the test will falsely claim an effect (reject the null hypothesis) α % of the time.

In experimental work a slightly different interpretation is given the results, and this is termed a "significance level" or "significance test." One computes the per cent of outcomes under the null hypothesis for which the absolute value of the differences is larger than the absolute value of the observed difference. This is the significance level, commonly called a p-value, and is interpreted as weight against the null hypothesis. The lower the value, the more weight against the hypothesis of no seeding effect. Hence one reads statements like, "With no effect one would only see larger differences p % of the time."

Estimation

From the beginning one of the primary quantities of interest has been the difference in the averages of the seeded and unseeded days calculated from all 30 days. As explained this quantity cannot be known exactly but only estimated. It turns out that in a technical sense the estimate obtained from the difference in the averages of the observed seeded and unseeded groups is unbiased. This means that the conceptual average value of differences computed from a long series of independent, conceptual repetitions of the experiment is exactly the value it is estimating. This is based solely on the fact that the observation is from one of a conceptual series of independent randomizations. It is for this same reason that the Gallup poll is unbiased.

If one is willing to assume the seeding effect is constant and additive, then estimates of the variability among the 30 days can be used to estimate how precise the original estimate is. In particular by inverting the hypothesis procedure one can obtain a confidence interval for the effect. (See Rao, p. 470).

Appendix C: Table of Randomized Rain Stimulation Experiments

The following table is an extension and slight modification of a table presented by Neyman and Scott in their paper for the 1967 Skywater Conference II. Their table categorized most of the known randomized experiments at that time, experiments involving seeding to affect rainfall or snowfall. This table deals only with those randomized experiments that examined seeding effects on rainfall. It also extends their original table by using results stated in NAS, 1973; Smith, et al., 1971; and Woodley, Jordan, et al., 1977. The experiments are classified into three groups based on the estimated seeding effects regardless of whether they were judged to be statistically significant. The first group are those whose results give an estimated positive seeding effect. Usually more than one analysis is performed for an experiment because of different definitions of the target area, measurement interval, and appropriate meteorological conditions. Group two are those experiments whose various analyses give both positive and negative estimated effects. Group three are those that give an estimated negative seeding effect.

Classification of Most Known Randomized Rain Stimulation Experiments According to the Indicated Effect of Seeding Whether Judged Significant or Not

Group 1: Experiments Indicating Increase in Rain Due to Seeding.

1. Snowy Mountains, Australia, 5 years beginning 1955.
2. New England, Australia, 6 years beginning 1957.
3. Delhi, India, 9 years beginning 1957.
4. Darling Downs I, Australia, 6 years beginning 1958.
5. Japan, 3 months in 1960.
6. Agra, India, 6 years beginning 1960.
7. Israel, 5 years beginning 1961.
8. Munnar, India, 2 years beginning 1964.
9. Shadehill, North Dakota, 2 years beginning 1965.
10. FACE, Florida, 5 years beginning 1970.

GROUP 2: EXPERIMENTS WITH SEVERAL APPRAISALS SOME INDICATING INCREASES AND SOME DECREASES IN RAIN DUE TO SEEDING.

1. ACN-WB, WASHINGTON-OREGON, 1.5 YEARS BEGINNING 1953.
2. NECAXA, MEXICO, 10 YEARS BEGINNING 1956.
3. SANTA BARBARA, CALIFORNIA, 3 YEARS BEGINNING 1957.
4. GROSSVERSUCH III, SWITZERLAND, 7 YEARS BEGINNING 1957.
5. LAKE ALMANOR, CALIFORNIA, 5 YEARS BEGINNING 1962.
6. TASMANIA, 4 YEARS BEGINNING 1964.
7. RAPID PROJECT, SOUTH DAKOTA, 3 YEARS BEGINNING 1966.

GROUP 3: EXPERIMENTS INDICATING DECREASE IN RAIN DUE TO SEEDING.

1. SCUD, EAST COAST U.S., 1.5 YEARS BEGINNING 1953.
2. ARIZONA I, 4 YEARS BEGINNING 1957.
3. SOUTH AUSTRALIA, 3 YEARS BEGINNING 1957.
4. WARRAGAMBA, AUSTRALIA, 4 YEARS BEGINNING 1959.
5. WESTERN QUEBEC, 4 YEARS BEGINNING 1959.
6. DARLING DOWNS II, AUSTRALIA, 1.5 YEARS BEGINNING 1960.
7. PROJECT WHITETOP, MISSOURI, 5 YEARS BEGINNING 1960.
8. ARIZONA II, 3 YEARS BEGINNING 1961.
9. FRANCE, 1.5 YEARS BEGINNING 1961.
10. RAPID PROJECT, SOUTH DAKOTA, 1965.

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